

Interactive comment on “Iodine chemistry in the troposphere and its effect on ozone” by A. Saiz-Lopez et al.

Anonymous Referee #1

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This paper reports a global modelling study of the abundance and impacts of iodine upon tropospheric chemistry and composition, based upon new insights into the sources of iodine species in the marine atmosphere, and current understanding of iodine oxide kinetics and photochemistry, effectively updating previous modelling studies from a couple of decades ago. The results show an important role for iodine chemistry in contributing to chemical ozone destruction in the troposphere, and a distinctive spatial distribution of atomic iodine in the mid-troposphere.

The manuscript makes an important integrative contribution to a topical area of atmospheric chemistry, providing quantitative assessments of iodine chemical impacts, while also communicating the uncertainty in iodine oxide photochemistry which limits more definitive assessments of its role. The use of two scenarios for I_xO_y photochem-

istry is appropriately conservative in this respect. There are a few areas where greater clarity is needed, and where further exploration of the uncertainties in the iodine source strength and photochemistry is warranted (outlined below); subject to revision addressing these comments, the paper is suitable for publication in ACP.

General Points

1 The new HOI/I₂ iodine source to the MBL is a function of ozone deposition, amongst other factors. Given that one finding of this work is a substantial role for iodine in chemical ozone depletion, there must be some feedback at work with climate / radiative forcing consequences. This aspect should be explored, at least qualitatively, in the discussion and conclusions.

2 The source strength values are critical to this study. How uncertain are these – it would be useful to indicate the range of values from e.g. the various reviews, and for the “new” I₂/HOI source, the confidence level in its magnitude. The statement on p. 19999 that without CH₃I iodine would have a negligible impact on the FT/UT seems to imply that the (very short lived) I₂/HOI source has no impact on iodine abundance or effects outside the MBL – can this be clarified ?

3 The authors do a good job of inclusion of many recent lab studies in their IO_x reaction scheme, but one reaction neglected appears to be IO + CH₃O₂, shown to have potentially substantial impacts for IO and to be competitive with (e.g.) IO + HO₂ and IO + NO₂ (e.g. Dillon et al. PCCP 2006). Is this a conscious choice ? What is the impact ? This reaction should be included in the relevant O₃ loss cycle (products dependent, which will increase the range of O₃ reductions).

4 p.19993/19994 it would be useful to give indicative values for the (pseudo) first order rate constants for I_xO_y loss through decomposition, uptake and photolysis to guide the account.

5 p.20004 A further possibility (besides (i) and (ii) mentioned) would be that I_xO_y do

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accumulate in the FT/UT. How do we know that this is not the case – are their e.g. aerosol composition data / iodate loadings which can be cited ?

6 The paper should reference Davis et al., JGR 101, 2135, 1996 as appropriate.

7 Higher iodine oxides are assumed to undergo rapid uptake to sea salt aerosol leading to a net sink. Is there evidence for this assumption ? (p.19992)

8 I_2O_4 spectrum – is there a need for allowance for solution shifts in the spectrum cf. gas phase ? The quantum yields / photolysis thresholds are not mentioned for $I_2O_2/I_2O_3/I_2O_4$.

Minor Comments

p.19988 L17+ needs rewording p.19996 L15 “of” not “on” p.19999 L16 I don’t think “notorious” is the right term p.20001 define WP p.20002 L12 some text missing after “during periods” p.20006 L28 “dawn & dusk” preferable to “twilight” which implies evening only p.20009 could usefully reference Solomon & Garcia JGR 1994 partitioning concept for Cl / Br efficiency at ozone depletion p.20010 line 6/7 phrasing needs attention p.20024 table caption is not accurate for I_xO_y – needs to reflect account in text.

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